theory of the atom; the basis of the fundamental hypothesis $\int p\,dq = nh$ is purely mathematical, and cannot be stated apart from mathematical conceptions. So also, I think, does the Maxwellian theory of the electromagnetic field, but to discuss this matter would lead us too far. All that I am concerned to assert is that there is no sense of the word "real," relevant to experimental physics, in which the principle of relativity has a different kind of influence on our views of what is real from any other theory. In particular, it has no influence whatever on the belief that matter is real in any scientific sense. It may have some bearing on that doctrine in the metaphysical sense; but since, after considerable philosophic reading, I am still unable to discover what metaphysicians mean by "real," I clearly cannot discuss that question. But since, again, I can understand science without understanding metaphysics, I am naturally convinced that the two are completely independent.

NORMAN R. CAMPBELL.

DR. NORMAN CAMPBELL has not understood me. Probably thinking that I am an idealist philosopher, he has supposed that I must be arguing that there is no scientific reality in the accepted meaning—that is, no scientific criterion of reality—and that the naturalist's mongoose, for example, has just as much or just as little reality as the drunkard's. What I was pointing out was the fact that the principle of relativity *is* the rejection of materialism. Materialism is a causal theory of scientific reality. It is the argument that when we pronounce anything in our sense-experience to be real we imply an independent cause of it. According to the principle of relativity, the inference is entirely unnecessary and to insist on it unscientific. Instead of this causal theory relativity offers a simple correspondence theory. The Minkowski-Einstein universe consists of events co-ordinated by observers in their different systems of reference. What is essential to constitute the "real event" of any observer is that there should be point-to-point correspondence between his co-ordination of it and the different co-ordinations of other observers. The co-ordination of an event by any observer—that is, his perspective of the event—is not an effect which is the appearance to him of a "causal" reality, but an actual case in point of the reality itself. The "event" in the four-dimensional continuum, and its track the "world-line," in re-forming the notion of scientific reality has relegated scientific materialism to its right place in the limbo of scholasticism. Whatever his disagreement, at least Dr. Campbell need not be alarmed for the basis of scientific research.

November 9. H. WILDON CARR.

Hybridity and the Evolution of Species.

I AM sorry to say that the postscript of "The Writer of the Article" to my letter on p. 274 in NATURE of October 27 is not according to facts. It was he who used Trillium, Dirca, and Scoliopus as evidence against "bad pollen" being an indication of hybridity; this evidence appeared to me to be insufficient, and I stated the reason why. In his postscript "The Writer of the Article" makes no attempt to refute my arguments against his view, but says: "In such cases as Trillium, Dirca, and Scoliopus it is not sufficient for him [meaning me] to suggest that they must be hybrids merely because they have bad pollen," though I have never suggested this, or referred to Trillium, Dirca, and Scoliopus in any of my previous writings.

Perhaps I may be allowed to make use of this occasion to state my point of view shortly with regard to the question of bad pollen. I do not think that bad pollen is proof of a hybrid origin, but consider it as "suspect"; neither do I share Jeffrey's view that absence of bad pollen is a sign of a non-hybrid origin; as a fact, I know that it is not. Some of my segregates of the cross *Antirrhinum glutinosum* × *majus* have bad pollen, while others have not. I further think that "The Writer of the Article" is mistaken in his view that the theory of mutation *requires* the occurrence of a certain proportion of defective germ-cells. The facts are these:—When de Vries found bad pollen in *Oenothera Lamarckiana* he accounted for its presence on the *assumption* that this defect was caused by mutations; we now know that *O. Lamarckiana* is a hybrid, so that it is much more probable that hybridity is the cause of the presence of bad pollen; recent cytological work seems even to prove this.

May I beg zoologists to answer a question I should like to put to them, namely: Is there any evidence that the presence of oligopyrene and apyrene sperms in some insects and molluscs is due to hybridity?

I might finish my remarks here were it not that "The Writer of the Article" reproaches me with "begging the question" at issue. Nothing is farther removed from my intentions, so that I desire to deal shortly with all the points mentioned by him. He argues that it militates against the general applicability of the origin of species by hybridisation that not all British roses are hybrids. I fail to see the force of this argument, as it is well known that homozygotes can arise from a cross without showing any sign whatever of their hybrid origin; consequently, the fact of specific purity can never be used as an argument against a hybrid origin.

Nor does the fact that pollen sterility and fertility behave as a pair of characters in the sweet pea and the velvet bean tell against the origin of that bad pollen by hybridisation, as "The Writer of the Article" seems to think, unless he can bring forward arguments in favour of his contention which are unknown to me. Until then I must acknowledge I fail to see how behaviour of a character already existing can reveal its mode of origin; the idea that specific characters do not segregate while varietal characters do is, of course, obsolete.

"The Writer of the Article" finishes his remarks by pointing out that it will be necessary to bring some more convincing argument in support of hybridisation as a constructive evolutionary factor before it is likely to receive much serious consideration from biologists. If he means some more convincing argument than the suggested hybrid nature of Trillium, Dirca, and Scoliopus—a suggestion which is not mine—I cordially agree with him. I wonder whether the following will assist him in taking a kinder view of my theory than he evidently does?

It is a generally acknowledged fact that new breeds of animals and plants can arise by crossing, while no other mode of origin of them has ever been proved, although various other modes have been suggested. We all know that Darwin explained the origin of new forms in Nature largely on the mode of origin of domesticated races, so that it is of considerable importance to know the real nature of the "variations" among plants and animals under domestication which play so important a *rôle* in Darwin's writings

Some time ago I happened to come across a letter of Darwin himself in his "Life and Letters," which seems to thrown important light on this momentous question. The letter is printed in full on p. 342 of the third volume of the "Life and Letters"; it was

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addressed to Dr. J. H. Gilbert, and dated "Down, February 16, 1876." The following passage is the one which interests us here:—

"It is admitted by all naturalists that no problem is so perplexing as what causes every cultivated plant to vary, and no experiments as yet tried have thrown any light on the subject. Now for the last ten years I have been experimenting in crossing and self-fertilising plants; and one indirect result has surprised me much, namely, that by taking pains to cultivate plants in pots under glass during several successive generations, under nearly similar conditions, and by self-fertilising them in each generation the colour of the flower often changes, and, what is very remarkable, they became in some of the most variable species, such as Mimulus, Carnation, etc., quite constant, like those of a wild species."

We now know that the colour changes and the becoming constant to which Darwin refers were the results of the repeated self-fertilisation of heterozygous material, so that the supposed variability evidently was nothing but segregation after a cross.

Velp, Holland, November 2. J. P. LOTSY.

IT would take too much space to reply in detail to all of Dr. Lotsy's statements, for which I have great respect. They go far outside the original point at issue, but it is necessary to refer to the more important of them, and it will then probably be seen that the others are immaterial. In his original letter (NATURE, October 27, p. 274), which commented on an article of mine on "British Roses and Hybridity" (NATURE, September 15, p. 99), he states that Jeffrey's work tends "to show that the presence of 'bad pollen' is *proof* [my italics] of a hybrid origin," and goes on to say that this view is "much strengthened" by other work. He correctly states that I took exception to that view, my own view being that "bad pollen" is unsafe as a criterion of hybridity, in support of which I cited various facts. As some of these facts were from a paper of which I was joint author, the original article was unsigned, but since this controversy, which was not of my seeking, has arisen, I prefer to sign my own name. In his present letter Dr. Lotsy seems to forget that the burden of proof rests upon those who assume that bad pollen is a proof of hybridity. He says that my postscript to his article is "not according to facts," and that he did not suggest the hybrid nature of Trillium, Dirca, and Scoliopus. I can only ask, if that is the case, why did he refer to them in his original article? Cytological work, which is by no means all "recent," proves that hybridity is *a* cause of bad pollen, but by no means proves that it is the only cause.

Dr. Lotsy has apparently omitted a consideration of lethal factors from his views. This is a more recent discovery which is of much significance in the interpretation of sterility, not only in Drosophila, but in various Œnothera forms, and it may apply either to gametes or zygotes. The conception has already been fruitfully applied, not only to various plants and animals, but also to man himself. When I said that the theory of mutation requires the occurrence of a certain proportion of defective germ-cells I had similar cases in mind, and did not mean to imply that mutations were necessarily *always* accompanied by germ-cell sterility. But clearly, if lethal factors account for germ-cell sterility in some cases, it is inadmissible to assume that bad pollen is in itself a proof of hybridity. We must apply the conception of multiple causes.

It follows that in any given case, such as that of *Oenothera Lamarckiana,* bad pollen *may* have originated from crossing, from lethal factors, or from some other cause, unless one or more of these possible causes can be eliminated. Dr. Lotsy says, "We now know that *O. Lamarckiana* is a hybrid." One can only ask how we know, and in what sense he is using the term hybrid. So far as the theory of mutation in Œnothera is concerned, it no longer matters whether *O. Lamarckiana* is a garden hybrid or not, since the work of Bartlett has proved that various close-pollinated wild American species of Œnothera show the same mutation behaviour.

Finally, I would say that if by his theory of evolution by hybridisation Dr. Lotsy means merely that the intercrossing of related races is the condition in which evolution has frequently taken place, I, for one, would heartily agree with him. For I have long advocated the view that among open-pollinated plants and most animals the evolutionary unit is an interbreeding population of closely related forms. I fancy many biologists adhere to a similar point of view. But I take it that Dr. Lotsy means much more than that by his theory, and if I understand him correctly, that is his reason for tacitly denying the existence of germinal changes. One can only ask how two visibly similar homozygous organisms when crossed can give rise to new germinal characters if they have not during the previous period of their isolation undergone germinal changes. R. RUGGLES GATES.

King's College, Strand, November 11.

Biological Terminology.

(*a*) "VARIATION is the sole cause of non-inheritance"; (*b*) "Apart from variations like exactly begets like, when parent and child develop under like conditions"; (*c*) "The development of the individual is a recapitulation (with additions and subtractions due to variations) of the evolution of the race." Here are three statements which seem to me "*in effect*" identical. To Dr. Bather the first two seem identical, but not the third. But if the child in his own development step by step recapitulates (with variations) the development of the parent, and the parent in the same way recapitulated that of the grandparent, and so on to the beginning, how, in the world, can the development of the individual be anything other than a recapitulation (with the accumulated variations) of the evolution of the race? If that be so, does not (*b*) necessarily involve (*c*)? (*c*) is merely (*b*) applied to a succession of parents and children. Dr. Bather says (NATURE, October 27, p. 271) that this is not what biologists mean. Then what do they mean? "Recapitulation" must be one of those terrible words which, like "inherit," are used, quite unconsciously, with a number of diverse and even contradictory meanings.

It is pleasant to find that Dr. Bather approves of Prof. Goodrich's address, for probably it has set the heather alight at last. In my humble way I also am enormously pleased. Still, Dr. Bather should bear in mind the history of this matter, some of which Prof. Goodrich indicates. As long ago as the 'eighties Weismann declared that "an organism cannot acquire anything unless it already possesses the predisposition to acquire it." At that time, too, doctors were beginning to insist that not actual diseases, but only predispositions to acquire them, were inheritable. Weismann failed to perceive the necessary consequences of his own idea—predisposition is all that can be inherited in the case of any character; all characters, therefore, are equally and in exactly the same sense innate, acquired, and inheritable. Instead he assumed, with Lamarck, that some characters are innate and others acquired, and so started the famous—or infamous—Lamarckian controversy. Sandeman did, however, very definitely

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